

## ***Interactive comment on “Local and remote temperature response of regional SO<sub>2</sub> emissions” by Anna Lewinschal et al.***

**Anonymous Referee #2**

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The authors conduct a variety of regional SO<sub>2</sub> emissions perturbation simulations in NorESM to calculate emissions-temperature metrics for several world regions. They increase emissions in each region by a factor that results in an equivalent global mean radiative forcing change, and decrease emissions to zero over Europe only to test the non-linearity of temperature response to SO<sub>2</sub> emissions. Overall, the methods are mostly sound and the results are interesting. I recommend publication subject to minor revisions.

Main comments:

1) It is worth citing and mentioning Conley et al. (2018), which looks at climate response to removal of US SO<sub>2</sub> emissions. There are some possibilities for comparison and discussion, such as their Table 3 which includes an estimate of temperature re-

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sponse per unit emission change of SO<sub>2</sub>.

2) Global maps of the temperature response to each of these SO<sub>2</sub> perturbations would strengthen this paper. Likewise I think global maps of ERF would be interesting as well. This would put the results in context of the few multimodel studies on this topic, such as the one cited above and Kasoar et al. (2016) which you mention already in the manuscript.

3) The biggest weakness of the paper is the use of a single coupled climate model, especially in a time when multimodel studies are becoming the norm. Ideally, the emissions-based RTP coefficients could be based on an average of several disparate models for more robustness. I understand that it's not feasible to do that in this study, but perhaps the authors could comment on whether or not they expect their results to be robust across additional CMIP models?

4) In Fig 4a and b, the error bars for just one standard deviation from the mean are quite large for the zero EU SO<sub>2</sub> emissions perturbation. How can the authors then be so sure about a nonlinearity in the response depending on the magnitude and sign of the emissions changes? Since the zero-out EU SO<sub>2</sub> emissions perturbation is much smaller in absolute magnitude than the 7xEU, you would likely need a slightly longer simulation than 160 years to reduce those error bars. Otherwise, I'm not sure how you can rule out the role of internal climate variability.

Other minor comments: 1) I'm not really seeing the grey shading in Figure 5? Is it there but just really small?

2) L332-333: this isn't a complete sentence. In general I think the phrase "e.g." is overused in this manuscript and seems to be rather unconventional to start sentences with that abbreviation which happens a couple of times here.

3) L84: "an comparison" should be "a comparison"

2018.

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